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**The Martial Origins of Democracy: A Global Study of Military Conscription  
and Suffrage Extensions Since the Napoleonic Wars**

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## **Abstract**

This paper examines the relationship between conscription (the compulsory enlistment of civilians for military service) and democracy. Using the best available cross-country comparable data on the history of conscription and democracy, we demonstrate that there is an empirical relationship between conscription and democratization, but the relationship is more complicated than commonly believed. Specifically, we find that conscription increases the likelihood of male suffrage extensions, but only in wartime (when the conscript army is mobilized). We find no relationship between conscription and democratization apart from extensions of the suffrage. Nor do we find support for the hypothesis that conscription shelters democracies from coups.

**Keywords:** conscription, democracy, suffrage, wars, coups

It is widely believed that conscription—the compulsory enlistment of citizens for military service—increases the likelihood of democratization, either because of an implicit contract between conscripted citizens and the state or because of the revolutionary threat posed by conscript armies. It is also widely believed that conscription decreases the likelihood of coups d'état, since volunteer armies are more easily coaxed into overthrowing democratically elected governments.

This paper uses the best available cross-country data on the history of conscription and the best available cross-country data on the history of democracy to examine the relationship between the two. We find that conscription increases the likelihood of suffrage extensions, but only during wars (when the conscript army is mobilized). We find no relationship between conscription and democratization beyond extensions of the suffrage, and we find no support for the hypothesis that conscript armies shelter democratically elected governments from coups d'état.

## **Soldiers, Citizens, and Democracy**

Conscription is one of the main methods states use to supply their armed forces with rank-and-file soldiers. The first modern conscript army was raised in France in the late eighteenth century. Today, the main alternative to conscription is a professional army; historically, many states depended on mercenaries (soldiers for hire).

An ideal-typical system of conscription has the following properties:

- a. *Military service is compulsory.* The law, or the constitution, provides

that military service is a duty.

- b. *Citizens serve.* Conscript armies are citizen armies. Only citizens—or, in some cases, prospective citizens—serve as conscripts.
- c. *Military service is temporary.* The conscript leaves his—or, occasionally, her—current vocation for a period of service in the military. (All men in ancient Sparta were soldiers. That does not mean that Sparta had a conscript army.)
- d. *Sanctions.* Eligible citizens who refuse to serve in the military are punished.<sup>1</sup>
- e. *No compensation, or only nominal compensation.* Although there are historical examples of states that have used the spoils of war to reward conscript armies, those are exceptions: conscripts became popular in nineteenth-century Europe precisely because they were *not* soldiers of fortune.

In other words, conscription is the forced recruitment of citizens for temporary military service with little or no compensation; in states that have introduced conscription, it is the primary military recruitment method, or at least a method on which states depend to meet their military manpower needs.

### ***Conscription and Democratization***

We wish to examine the idea that conscription is more democratic than other methods of military recruitment—such as a professional army—since it increases the likelihood of democratization and/or reduces the likelihood of democratic

breakdowns.<sup>2</sup>

It is possible to distinguish between two different mechanisms in the arguments scholars have made about the democratizing effects of mass conscription (we discuss the problem of democratic breakdowns in the next section).

The first mechanism can be called the *quid-pro-quo mechanism*. The idea is that citizens only treat laws that compel them to serve in the military as legitimate if they are given the right to vote.<sup>3</sup> Not coincidentally, this mechanism is similar to the basic mechanism in fiscal-contract theories of political regimes (treating political representation as a form of compensation for taxation). The reason is that conscription can be understood as a form of in-kind taxation. A state has two alternative ways of recruiting soldiers: it can either rely on compulsory labor (conscription) or hire workers at market wages (a volunteer army). Both strategies rely, in a general sense, on the state's ability to raise taxes.<sup>4</sup>

Extending voting rights is not the only concession that governments can make to compensate those who risk their lives for their country. It is well-known, for instance, that many welfare-state programs were originally introduced to support soldiers and former soldiers. As Theda Skocpol notes in *Protecting Soldiers and Mothers*, the first large-scale public pension system in the United States provided benefits for Civil War veterans and their families.<sup>5</sup> Much later, after the Second World War, the G.I. Bill in the United States provided for the veterans of another armed conflict. And government expenditure is not all. Kenneth Scheve and David Stasavage have recently shown that the large increases in progressive income taxes and other taxes on the rich during the twentieth century occurred precisely

because governments sought to compensate soldiers and veterans for the sacrifices they made in the First and Second World Wars. The logic behind these arguments about wars, benefits and taxes is similar to the logic behind our quid-pro-quo argument about conscription and political rights. The arguments are complementary, not conflicting.

The second mechanism we wish to consider can be called the *mobilization mechanism*. It is based on the idea that democratization is a response to popular mobilization and the threat of revolution.<sup>6</sup> Conscription brings together large numbers of soldiers that can easily be mobilized against rulers. Unless the government enjoys a high level of legitimacy, those armed masses might revolt.<sup>7</sup>

Many of the observable implications of the quid-pro-quo argument and the mobilization argument are identical, but there are two important differences, allowing us to test the relative explanatory power of the two theories.

First of all, the quid-pro-quo argument implies that conscription should be associated with one particular type of democratic reform: the extension of the male suffrage. The mobilization argument, by contrast, implies that conscription should also be associated with other democratic reforms, including the introduction of elections and the termination of monarchical control over government formation.

According to the quid-pro-quo argument, there is no relationship between conscription and democratization *in general*; what drives the relationship between conscription and the right to vote is a desire to overcome the perceived injustice of forcing some citizens to serve in the military without granting them the same political rights other citizens enjoy. Unlike other aspects of democratic reforms, the

right to vote is a concrete and tangible right that can be extended selectively to groups that are eligible for conscription, just as the Civil War pensions and the G.I. Bill benefits, which were also concrete and tangible and could be extended selectively. In fact the Civil War era in the United States provides a clear example of the quid-pro-quo mechanism behind the relationship between conscription and extensions of the male suffrage. In the 1860s, aliens in the United States who formally declared their intention to become citizens were given the right to vote, but they also became eligible for conscription.<sup>8</sup>

Second, both the quid-pro-quo argument and the mobilization argument suggest that the effect of conscription should be conditional on war, but not in identical ways. The quid-pro-quo argument implies that the effect of conscription should be greater *during* a war, since the sacrifice made by the conscripts is much greater when their lives are at risk. The mobilization argument implies that the effect of conscription should be greater in the *aftermath* of a war, since the revolutionary threat is greatest when the war is over and the soldiers come home.<sup>9</sup>

The reason that the revolutionary threat is most acute when the war is over is that the troops are no longer tied up in fighting. As long as the war is in progress, soldiers cannot effectively be turned against their own government, for if they abandon their posts, they are likely to find themselves powerless against their foreign foe even if they manage to overthrow the government. According to the mobilization argument, there is thus a critical period between the end of hostilities and the completion of demobilization in which the potential threat to the regime is greatest.



On the basis of the quid-pro-quo argument, we formulate the following hypothesis:

1. Conscription increases the likelihood that the male suffrage is extended, especially during a war.

On the basis of the mobilization argument, we formulate the following hypothesis:

2. Conscription increases the likelihood of a transition from authoritarianism to democracy, especially immediately after a war.

### ***Conscription and Coups***

We now turn to the argument that a democratic regime, once established, is less likely to descend into authoritarianism if its army is made up of conscripts as opposed to professional soldiers. The idea behind this argument is that when the military is composed of ordinary citizens, it is difficult for political and military leaders to seize and hold political power by undemocratic means.<sup>10</sup> “There must not be a large standing army subject to the behest of a group of schemers,” George C. Marshall wrote in his report to the U.S. Congress on how the Second World War was won. He concluded, “The citizen-soldier is the guarantee against such a misuse of power.”<sup>11</sup> For most of the history of post-Second-World-War (West) Germany, this idea of the citizen-soldier—or “citizen in uniform,” *Staatsbürger in Uniform*—was the basic principle of military organization.<sup>12</sup>

One way to formulate the hypothesis that follows from this argument is:

3. Conscription reduces the likelihood of transitions from democracy to

authoritarianism.

But it is possible to be more precise, for the specific causal mechanism behind the argument that conscription makes democracy more stable is that a professional army renders democracies vulnerable to the actions of “groups of schemers” (as Marshall put it). This suggests that conscription makes coups d’état less likely, not other forms of transition to authoritarianism. Hence:

4. In democracies, conscription reduces the likelihood of coups d’état.

### ***Contributions to the Literature***

Adam Przeworski has studied historical suffrage extensions in a wide range of countries, arguing that the main driver behind these reforms was the threat of revolution. He has also explored a version of the quid-pro-quo argument, but without estimating the effect of conscription (he instead framed the issue as one of elites preparing for war).<sup>13</sup> Similarly, Aidt and Jensen have studied implicit popular threats as potential drivers behind suffrage extensions, again alluding to the conscription argument without studying it explicitly (Aidt and Jensen simply introduced year dummies in their statistical models).<sup>14</sup>

The only study we have found that engages in a direct, quantitative analysis of the effects of conscription on democracy is Stephan Pfaffenzeller’s article “Conscription and Democracy.”<sup>15</sup> Pfaffenzeller’s study is mainly concerned with the case of Germany, but also engages in a comparative analysis of the relationship between conscription and democratic stability and of the relationship between conscription and coups d’état. Pfaffenzeller relies on the same data that we use in

this paper, but limits almost all of the analysis to cross-sectional comparisons as opposed to comparisons over time. The findings are entirely negative: Pfaffenzeller claims that conscription has no effect on either democracy or democratic stability. In this paper, we use panel data, not just cross-sectional data, and this allows us to show, contrary to one of Pfaffenzeller's findings, that a central hypothesis about the relationship between conscription and democracy does have empirical support: conscription increases the likelihood of male suffrage extensions, albeit only in times of war.

In another recent paper, Antonis Adam hypothesizes that what he calls “unstable” democracies are more likely to introduce conscription than stable democracies, the reason being that unstable democracies adopt conscription strategically to reduce the risk of coups.<sup>16</sup> (Stable democracies have no need to protect themselves in this way, Adam argues; they consequently have no need to introduce, or retain, conscription.) In this paper, we test the conscription-coup relationship directly, not indirectly. Victor Asal et al., in their recent paper “I Want You!,” also find that democracies are less likely than autocracies to *introduce* conscription. This will be an important potential confounder to consider in our empirical analysis, to which we now turn.<sup>17</sup>

## **Data and Methods**

We rely on historical data on military conscription collected by Nathan Toronto.<sup>18</sup> Toronto's dataset provides information about whether conscription was “the prin-

cial means for satisfying the military manpower requirement” of a country. His coding is based on whether “a non-trivial number of recruits are enlisted through force.” This definition is similar to other definitions in the literature. When examining the difference in combat casualties between states using volunteer armies and conscripted armies, for example, Vasquez codes “[c]ountries with armies composed mainly of conscripts” as conscript armies.<sup>19</sup> Since the definition that we proposed in the introduction is “the forced recruitment of citizens for temporary military service,” with the addition that in states with conscription, “this is the primary military recruitment method, or at least a method on which they depend to solve their military manpower requirements,” Toronto’s measure comes close to what we are after. To make sure of this, we conducted an independent test to validate Toronto’s coding, based on a selection of thirteen countries that display a mixture of suffrage extensions and military recruitment methods as well as providing broad regional spread. By and large, our assessment agreed with Toronto’s. We did find that it is sometimes hard to decide what should be considered “a non-trivial number of recruits.” This vagueness introduces some noise when recording the exact date of the introduction of conscription, but it should not bias the data in any particular direction.<sup>20</sup>

Our measure of franchise extensions comes from Adam Przeworski and his collaborators, who have coded, for each country and year in their sample, (1) if “suffrage was extended by class,” (2) if “suffrage was extended by gender,” and (3) if “suffrage was extended by both class and gender.”<sup>21</sup> In our main analysis, we only include extensions of the male suffrage (1 and 3), since there are very few

examples of female conscription in our sample. In a second set of analyses, we examine extensions of the female suffrage (2) as a “placebo” test, showing that conscription has no effect on female suffrage *per se*. This pattern—an effect on male suffrage, at least when the army is mobilized, but no discernible effect on female suffrage—is exactly what one would expect to find on the basis of the quid-pro-quo hypothesis. We also include only those extensions that incorporated additional socioeconomic groups in the electorate (“by class,” as opposed to ethnic or religious groups).

Our measure of democracy comes from Boix et al., who code a country as democratic if “[t]he executive is directly or indirectly elected in popular elections and is responsible either directly to voters or to a legislature,” “[t]he legislature (or the executive if elected directly) is chosen in free and fair elections,” and “[a] majority of adult men has the right to vote.”<sup>22</sup> Our measure of coups comes from Svobik, who categorizes a democratic reversal as a coup if “the armed or security forces participated in the removal a democratically elected government by employing or threatening violence.”<sup>23</sup> Data on wars and the end of wars are taken from the Correlates of War dataset, in which hostilities are coded as wars if there was sustained combat, involving organized armed forces, that resulted in at least 1000 battle deaths over a twelve-month period.<sup>24</sup>

We control for several potential confounders. There is broad agreement in the literature that apart from war itself, socio-economic modernization is a driver of both suffrage extensions and military conscription.<sup>25</sup> We therefore control for the natural log of GDP per capita and a measure of urbanization, defined as the pro-

portion of the population living in cities.<sup>26</sup> To account for short-term economic crises—which increase the likelihood of both democratization and democratic breakdowns—we control for GDP-per-capita growth.<sup>27</sup> To account for the argument that suffrage extensions are primarily related to income inequality, we include a measure of (rural) inequality: family farms as a percentage of total cultivated area or of total area of holdings.<sup>28</sup> Finally, natural resource dependence arguably obstructs the quid-pro-quo logic, cutting governments loose from having to rely on citizen conscripts (and, therefore, from having to make suffrage concessions).<sup>29</sup> We therefore control for resource dependence, which is defined as the percentage of government revenues from oil and the percentage of government revenues from minerals.<sup>30</sup> To provide at least a simple check for reverse causation, we lag all explanatory variables one year.

We employ the same set of control variables in our analyses of transitions to democracy and our analyses of democratic survival (and the risk of coups). This approach has support in the literature: socioeconomic modernization, for example, is a strong predictor of democratic survival, and economic crises have been found to trigger democratic breakdowns.<sup>31</sup> One set of control variables that is typically added to models of democratic survival concerns the institutional organization of democracies (such as whether the system of government is presidential or parliamentary). Since recent evidence indicates that institutions do not matter for democratic survival, and since we lack reliable measures of institutions covering our full sample, we refrain from controlling for these features of democratic institutions; instead, we prefer a symmetrical strategy, using the same control variables

in all models.<sup>32</sup>

Since all our outcome variables are binary, we rely on logit models that include a polynomial that controls for the time that has passed since last event (extension, transition, or coup) and a control for the number of previous events (or, in the case of transitions to and from democracy, the number of previous years with democracy or authoritarianism). This is a standard approach in the literature on methods for time-series cross-section data with a binary dependent variable.<sup>33</sup>

In most of our models, the first observation is from 1817 (since we only have data on wars from 1816) and the last observation is from the early 2000s (the year 2000 for the models with all control variables included).

## **Evidence**

In this section, we evaluate the four hypotheses that we formulated in Section 1.

### ***Male Suffrage***

We begin with the relationship between conscription and extensions of the franchise. In total, we have data for 9480 country-years, but since it is not meaningful to examine franchise extensions when the franchise has already been extended to all males—as we expect conscription to influence the extension of the franchise to males only—we exclude all country-years in which the franchise had already been fully extended to the adult male population. This leaves us with only 1371 country-year observations (some countries, such as the United States, that at

least on paper extended the franchise to all males before Toronto's data series begin are consequently not included in the analysis).<sup>4</sup> We observe 47 extensions of the franchise (to men only or to men and women, in both cases for socioeconomic reasons only). In other words, an extension of the franchise happened in approximately 3 percent of all cases. Table 1 provides information about descriptive statistics for all variables in all our models, concentrating on the samples for which we have data on all control variables. In the Online Appendix (Tables A2–A6), we list all the countries that are included in our analyses. We also provide details on the periods that are covered by our data on each country. The sample is unbalanced (since countries enter the sample when they become independent or when our data series begin and leave the sample when all men have the right to vote). But we observe franchise extensions over most of the time period that we consider – the first extension in our analysis occurred in 1834 (in Portugal) and the last occurred in 1985 (in Brazil). In other words, our findings are not driven by extensions that occurred in any particular historical period.

Our first set of statistical results (in column 1 in Table 2) show that there is no *unconditional* relationship between conscription and extensions of the franchise. At first glance, one might suspect that part of the reason for this is that democracies are less likely to introduce conscription in the first place.<sup>34</sup> We have two reasons not to worry about this particular kind of endogeneity bias, however. First, since only countries that have not yet extended the suffrage are included in the model, only *partially* democratized countries enter our estimation sample. Second, by lagging the conscription variable one year, as well as controlling for duration



dependence and the number of previous extensions, the causal path that runs from suffrage to conscription should be blocked. We therefore believe that column 1 shows a true null finding: all else equal, countries with military conscription are *not* more likely to extend the suffrage.

But all is not always equal. According to Hypothesis 1, the relationship between conscription and franchise extensions only exists in times of war, when the sacrifice that (male) citizens are asked to make is the greatest. We must therefore estimate a model that includes a measure of war and an interaction term (Conscription  $\times$  War) (column 2). The coefficient for the interaction term is clearly significant and larger in magnitude than the sums of the coefficients of the constitutive terms, so there are strong reasons to believe that the conditional relationship that we expected exists. In other words, we find preliminary support for Hypothesis 1. We next include our set of control variables (column 3), which does not change the results: having conscription and having a war at the same time is still associated with an increased likelihood of franchise extensions. To give a sense of the substantive meaning of the estimates, we have calculated, on the basis of the model in column 3, the predicted probability of an extension of the franchise as a function of conscription and war (and the combination of the two), holding all other variables in the model at their observed levels and then calculating mean predicted probabilities for all observations in the sample.<sup>35</sup> The results are unequivocal (see Figure 1). *Without* conscription, the predicted likelihood of an extension of the franchise (to men) in a given year is approximately 1.5 percent, regardless of whether there is a war or not. *With* conscription, the predicted likeli-

hood is 2.3 percent in peacetime and 11.9 percent in times of war. The difference between the conscription-and-war scenario and all the other three possible scenarios is statistically significant at the  $p < 0.01$  level (but there are no statistically significant differences among the three other scenarios). This suggests that there is considerable support for our first hypothesis: when a conscript army fights a war, there is an especially high likelihood that the male suffrage is extended to larger groups.

In the online appendix (Table A1), we include a number of tests of the robustness of this finding. First, since the war variable is lagged, franchise extensions occurring the year after the end of a war are also included. Since this could thus also capture the effect of a war ending, more in line with the mobilization hypothesis, we also included the war dummy unlagged as a robustness check (model 1 of Table A1), as well as two models where we instead simply control for whether a war ended at  $t-1$ , with (model 3) and without (model 2) covariates. Second, we also include tests examining the *introduction* of conscription. Since these are rare events, we cannot estimate the model with the war interaction if we only lag this introduction variable one year, so instead we introduce a variable coding whether conscription was introduced in the last five years. Models 5–7 in the online Appendix table (A1) then replicate models 1–3 in Table 1, using this introduction variable. Third, although we include the usual suspects among the covariates, one can always argue that unobserved country characteristics may be driving our results. To control for this we introduce country-fixed effects, forcing us to turn from logistic regression to a linear probability model, with the 1-year lag (model 4

of Table A1) and the 5-year introduction variable (model 8).

In all these robustness tests, the interaction between conscription and war remains statistically significant and largely of the same magnitude. The linear probability models also indicate a substantial interaction effect that is similar to the predictions from the logistic regressions.

Let us now provide some case-study flesh on these statistical bones. The connection between conscription and the politics of franchise extensions is evident in both German and British history. As Janowitz and Burk have observed, the German Chancellor Otto von Bismarck extended the franchise to all men when Germany was unified in 1871, following the Franco-Prussian War.<sup>36</sup> All men in the new united Germany were required to serve in the military. In Britain, by contrast—which relied on a blend of feudal arrangements and mercenary soldiers—the franchise was only extended gradually during the nineteenth century (through the Second Reform Act of 1867 and the Third Reform Act of 1884). It was not until the outbreak of the First World War that military service became obligatory, and it was only toward the end of the war that the franchise was extended to all men. The extension of the franchise in Great Britain was only gradual, in comparison with Germany, which is arguably an effect, at least in part, of the differences in manpower policies. The German and British cases thus demonstrate the relevance of examining the development of franchise extensions through the lens of conscription.

### ***Female Suffrage***

We next try the placebo test that we mentioned earlier, asking if conscription (alone or in combination with war) leads to a higher likelihood of an extension of *female* suffrage (Table 3). We don't expect that conscription has this effect, since there are no cases of states with extensive female conscription in our data, so if we did find such a relationship, there would be strong reasons to fear that the relationship between conscription and the extension of male suffrage that we have just discussed is spurious. As before, descriptive statistics for this analysis can be found in Table 1 and details on the sample can be found in the Online Appendix.

What we find, however, is that conscription is *not* related to the extension of female suffrage. In fact, in the two first models in Table 3, we find a *negative* relationship (which is significant during peace years). With the full-control model, this relationship vanishes, however. This increases our confidence that there is a causal relationship between conscription and extensions of the male franchise in wartime (even if the possibility that a relationship is spurious can never be completely ruled out when relying on observational data).

A concrete example of an extension of the male suffrage without a corresponding extension for females is the introduction of universal suffrage for men over 25 in the German Empire in 1871, immediately after the Franco-Prussian war of 1870–1871.<sup>37</sup> This extension of the franchise, which we have already mentioned in connection with the comparison between Germany and Britain, followed the spread of conscription among the states involved in the war, states that would later be included in the German Empire in 1871.<sup>38</sup> The *women* of the German Empire, on the other hand, did not get the right to vote until 1918, when the German

Empire collapsed and was replaced by a republic.

It is important to note that while we did not find a relationship between conscription and female suffrage extensions (defined as extensions *only* affecting females), women have sometimes been given the right to vote in connection with *general* franchise extensions that were associated with conscription. In Canada in 1917, for example, the need for conscripts to fight in the First World War, combined with a lack of popular support for conscription, resulted in the Military Voters Act of 1917, which extended the franchise to all serving members of the Canadian Armed Forces, male or female, independent of residency requirements.<sup>39</sup> This meant that female nurses as well as Indian persons and those under 21 became eligible to vote (illustrating the quid-pro-quo mechanism).

As previously mentioned, a similar development took place in the United Kingdom in 1918, when the Representation of the People Act of 1918 was given royal assent, abolishing the property requirements for male voting. In addition, this piece of legislation extended the franchise to women over 30 years of age that met minimum property requirements. The main reason that this extension of female suffrage was passed in 1918 was the role that women had played—and continued to play— in supporting the war effort.<sup>40</sup>

### ***Democratization***

We next turn to Hypothesis 2, and, consequently, to the relationship between conscription and transitions to democracy (Table 4). We have data on conscription and democracy for 10357 observations. Since we are only interested in transitions

to democracy, however, we only consider country-years when the regime was authoritarian in the previous year. This leaves us with 6317 observations. We assess the relationship between conscription in year  $t - 1$  and a transition to democracy between year  $t - 1$  and year  $t$ . Here we find no discernible relationship at all (column 1). In other words, there is no support for the idea that there is an unconditional relationship between conscription and democratization. Moving on to column 2, we proceed to test whether there is an effect of conscription on democratization in the aftermath of a war. We find that there is not. The coefficient is positive, as expected, but it is far from being statistically significant. That also applies to column 3, where the battery of control variables that we discussed earlier is included.<sup>41</sup>

It may seem surprising that we find a conditional relationship between conscription and extensions of the franchise but no relationship at all between conscription and democracy, since the measure of democracy that we rely on includes a franchise threshold.<sup>42</sup> However, a closer inspection of the cases included in our analysis reveals that only three transitions to democracy in the sample of countries for which we have franchise data occurred in the same year as an extension of the franchise (as measured by Przeworski).<sup>43</sup> It appears, therefore, that most of the transitions to democracy in the Boix et al. dataset were associated with other sorts of institutional changes (such as change in the relationship between the legislature and the executive).

### ***Breakdowns and Coups***

So far, we have found support for one hypothesis: the qualified quid-pro-quo hypothesis, Hypothesis 1, which states that conscription increases the likelihood of democratization during wars. We have found no support for Hypothesis 2. This suggests that the theory that best accounts for the relationship between conscription and *democratization* is a quid-pro-quo argument about an implicit contract between conscripted citizens and their political leaders.

We now discuss the remaining hypotheses, Hypotheses 3 and 4. The findings can be summarized briefly. As the results in Table 5 show, there is no relationship at all between conscription and democratic stability. There is no effect of conscription on the likelihood of transitions to authoritarianism (columns 1 and 2) or on coups (columns 3 and 4). We once again remind the reader that descriptive statistics for this analysis can be found in Table 1 and detailed information about the sample can be found in the Online Appendix. A concrete example of the inability of conscription to prevent coups is Argentina, which had conscription for most of the twentieth century (conscription was formally abolished in 1994), but which nevertheless experienced several military coups during the same time period (including the coups of 1930, 1955 and 1976, all of which overthrew democratically elected governments). In the same region, conscription did not prevent the 1964 military coup in Brazil or the coup in Bolivia in the same year. Outside of Latin America, we find similar developments in Turkey in 1960, 1971 and 1980, and in Greece in 1967 (to mention but a few examples).

The conclusion must be that conscript armies are no less likely to stage coups

than their professional counterparts. Several explanations can be given for this outcome. The idea that conscription is likely to lessen the risk of coups in democracies is based on two assumptions. The first assumption is that conscripts are opposed to coups against democratically elected governments. This is not necessarily the case. The coup d'état in Chile in 1973, for example, was carried out with the help of conscripts. The attempt by elements within the French Army to overthrow Charles de Gaulle in 1960 did fail because of the refusal of conscripts to obey their immediate superiors, but our findings suggest that this was an exception, not the rule.<sup>44</sup> The second assumption is that conscripts are able to coordinate their refusal to participate in a coup. We believe that this assumption is based on a mistaken idea of modern military organizations, for in reasonably well-functioning organizations, such coordination is likely to be difficult.

Moreover, the thesis that a conscript army is less likely to participate in a coup d'état ignores some of the distinctive characteristics of the conscript system. A system with conscription typically requires a large and professional officer corps. In contrast to a standing army, where military competence is spread throughout the hierarchy, the military competence in a conscripted army rests only in the hands of the officers.<sup>45</sup> It seems likely, therefore, that the disposition of the officer corps, or their "ethos" to use Nassif's wording, is a much more important factor than the method of recruitment.<sup>46</sup> In other words, the probability of a coup d'état depends on the overall democratic maturity of the officer corps, not on whether the country relies on conscripted soldiers or paid professionals.



## Conclusion

In this paper, we have used the best available cross-country comparable data on the history of conscription and the best available cross-country comparable data on the history of democracy to conduct a quantitative analysis of the relationship between conscription and various measures of democracy. The results are mixed. One central hypothesis—the idea that more men get the right to vote when men are conscripted and risk having to give up their lives for the state—receives qualified support: there is such a relationship, but only in times of war, when the risk that conscripted soldiers face is greatest. Other hypotheses receive little support. We find no relationship between conscription and democratization *beyond* extensions of the suffrage, and we find no support for the hypothesis that conscript armies shelter democratically elected governments from coups d'état.

Many studies have neglected to take the severity of the risks that conscripts face into account. Our results suggest that conscription is only associated with extensions of the suffrage when the risk of death or serious injury becomes tangible.

The quid-pro-quo argument overlaps in part with the argument behind the mobilization hypothesis, but there are clear differences. If the threat of revolution were the key explanation behind the relationship between conscription and democracy, we would have expected to find an empirical relationship between conscription and broad democratic reforms. What we find, however, is that conscription is only associated with extensions of the male suffrage. Throughout history,

the franchise has sometimes been extended to men in order to enhance the legitimacy of the institution of conscription—and to mobilize the population in situations when those who were called to serve knew that they would face great danger.

The implications are clear. Conscription is not a democratizing institution in and of itself. Nor does it provide democratic governments with protection against coups. But it sometimes leads to the enfranchisement of those who are required to fight.

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<sup>1</sup> There have always been exceptions. In the nineteenth century, married men, breadwinners, the mentally ill, and the physically disabled were typically exempt. It was also possible, in many countries, for wealthy individuals to avoid service. In the twentieth century, the main exemptions concerned conscientious objectors and the mentally or physically disabled.

<sup>2</sup> The more general question of the military's role in democracies and in democratization has spawned a large literature. In the spring of 2017, for example, this journal had a special issue on the impact of militaries on democratization (vol. 24, number 5). Some scholars have noted that military interventions in dictatorships have sometimes serve democratization well, such as when the conscript-based military in Portugal toppled the authoritarian *Estado Novo* in 1974 and helped the country transition to democracy (see Kuehn, 'Midwives or Gravediggers', 783–784). For a different view, see Luckham, also in this journal, who believes that the relationship between democracy and the military is "inherently problematic" ('Democracy and the Military', 1). The idea that *some* forms of military organization are inherently more democratic than others goes back to classical thinking. In his *Politics* Aristotle notes, for example, that "light armed troops, and the navy are wholly in the side of democracy" (quoted in Deudney, *Bounding Power*, 97–98). The link between military organization and popular government is also evident in the constitution of the Roman Republic. In one of Rome's four popular assemblies, the *comitia centuriata*, citizens voted as part of their military units, the *centuriae*. The political influence of the citizens corresponded directly to their types of armaments. The poorest strata were assigned to the *capite censi*, who did not have the right to bear arms, and who had an extremely weak political position vis-a-vis other Roman citizens (Le Glay et al., *A History of Rome*, 32). Much later, in the sixteenth century, Niccolò Machiavelli, in *The Prince*, regarded citizen-soldiers as superior to mercenaries because of the bond it fostered between the rulers and the ruled. Rulers depended on the citizenry to wage wars, and in return for their pledge to fight citizens were rewarded with property rights and access to legal forums (see Holmes, 'Lineages of the Rule of Law'). Concerning the nineteenth century, Tullock claims in his *Autocracy* that the "composition of the armies" was one of the "pro-democratic aspects" of the period between the French Revolution and the early twentieth century. "[A] large scale conscript army does have certain democratic overtones," Tullock notes. "A ruler does have to maintain a certain minimum amount of popular support if that conscript army is to fight for him. In this respect, it is different from a professional army, which may be hired for the specific purpose of keeping the common man down." (Tullock, *Autocracy*, 72.)

<sup>3</sup> For the theoretical rationale behind this idea, see especially Levi, *Consent, Dissent and Patriotism*.

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- <sup>4</sup> Poutvaara and Wagener, 'To Draft or Not', 1.
- <sup>5</sup> Skocpol, *Protecting Soldiers and Mothers*. For a more recent survey of the literature on the relationship between war and the welfare state, see Obinger and Petersen, 'Mass Warfare and the Welfare State.'
- <sup>6</sup> Many scholars, including Boix, in *Democracy and Redistribution*, and Acemoglu and Robinson, in *Economic Origins*, treat the threat of revolution as a key mechanism in processes of democratization.
- <sup>7</sup> Walzer *Obligations*, 153-170.
- <sup>8</sup> Ibid., 107.
- <sup>9</sup> Therborn, 'The Rule of Capital'.
- <sup>10</sup> Janowitz and Burk, *On Social Organization*; Burk, 'The Citizen Soldier'.
- <sup>11</sup> Marshall, *The Winning of the War*, 117.
- <sup>12</sup> Frevert, *A Nation in Barracks*, Sec. 5:3.
- <sup>13</sup> Przeworski, 'Conquered or Granted?', 293.
- <sup>14</sup> Aidt and Jensen, 'Workers of the World'.
- <sup>15</sup> Pfaffenzeller, 'Conscription and Democracy'.
- <sup>16</sup> Adam, 'Military Conscription'.
- <sup>17</sup> Asal et al., 'I Want You!'.
- <sup>18</sup> Toronto, 'Military Recruitment' and *Why War is Not Enough*. See also Asal et al., 'I want You!'. Toronto's data are available as replication files for Asal et al., 'I want You!'.
- <sup>19</sup> Vasquez, 'Shouldering the Soldiering', 858.
- <sup>20</sup> The countries we examined were Portugal, Brazil, Japan, United Kingdom, Turkey, Hungary, Tunisia, South Africa, Iran, Kuwait, Costa Rica, Tanzania, and Bahrain.
- <sup>21</sup> Przeworski, 'Political Institutions'.
- <sup>22</sup> Boix et al., 'Dataset of Political Regimes'.
- <sup>23</sup> Svobik, 'Which Democracies will Last?', 730.
- <sup>24</sup> Sarkees and Wayman, *Resort to War 1816-2007*.
- <sup>25</sup> Ticci and Vindigni, 'War and Endogenous Democracy'; Przeworski, 'Conquered or Granted?'; Aidt and Jensen, 'Workers of the World'; Asal et al., 'I want You!'.
- <sup>26</sup> Maddison, 'Statistics on World Population'; Banks, 'Cross-national Time-series'.
- <sup>27</sup> Haggard and Kaufman, *Political Economy of Democratic Transitions*; Gasiorowski, 'Economic Crisis'; Remmer, 'The Sustainability of Political Democracy'; Przeworski et al., *Democracy and Development*; Maddison, 'Statistics on World Population'.
- <sup>28</sup> Boix, *Democracy and Redistribution*; Acemoglu and Robinson, *Economic Origins*; Vanhanen, 'Democratization and Power Resources'.
- <sup>29</sup> Ross, 'Does Oil Hinder Democracy?'.
- <sup>30</sup> Haber and Menaldo, 'Do Natural Resources Fuel Authoritarianism?'.
- <sup>31</sup> Przeworski et al., *Democracy and Development*; Persson and Tabellini, 'Democratic Capital'; Houle, 'Inequality and Democracy'; Alemán and Yang, 'Duration Analysis'; Haggard and Kaufman, *Political Economy of Democratic Transitions*;

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Bernard et al., 'Economic Performance'; Svolik, 'Authoritarian Reversals and Democratic Consolidation'.

<sup>32</sup> Cheibub, *Presidentialism, Parliamentarism and Democracy*; Sing, 'Explaining Democratic Survival Globally'.

<sup>33</sup> Carter and Signorino, 'Back to the Future'; Beck et al., 'Taking Time Seriously'.

<sup>34</sup> See Asal et al., 'I want You!'.

<sup>35</sup> Muller and MacLehose, in 'Estimating Predicted Probabilities from Logistic Regression,' show that this method of calculating predicted probabilities, which they call "marginal standardization," is superior to other methods, noting that it is "the appropriate method when making inference to the overall population" and that other methods "should be used with caution, and prediction at the means should not be used with binary confounders."

<sup>36</sup> Janowitz and Burk, *On Social Organization*, 228.

<sup>37</sup> Deutscher Bundestag, 11.

<sup>38</sup> Förster, 'Dreams and Nightmares', 349.

<sup>39</sup> Elections Canada.

<sup>40</sup> Ogg, 'The British Representation', 500. Introducing the Bill, Home Secretary George Cave said: "War by all classes of our countrymen has brought us nearer together, has opened men's eyes, and removed misunderstandings on all sides. It has made it, I think, impossible that ever again, at all events in the lifetime of the present generation, there should be a revival of the old class feeling which was responsible for so much, and, among other things, for the exclusion for a period, of so many of our population from the class of electors. I think I need say no more to justify this extension of the franchise." After the war, in another clear illustration of the quid-pro-quo motivation for this extension of the franchise, David Lloyd George, who served as Prime Minister when the Act was passed, stated the following: "The Conscription Act converted the country to a realisation of the injustice of this state of things. Millions of men were forced to risk their lives for a policy which they had no share in fashioning. Millions of women faced anxieties and tortures worse than death in pursuit of the same policy, and yet no woman was allowed to express any opinion as to the selection of the rulers who led them to this sacrifice. It was felt to be so unjust that in the exaltation of war, which lifted men to a higher plane of equity, this obvious wrong was redressed. Hence the greatest of all the Enfranchisement Acts, the Act of 1917 [sic!], that for the first time converted the British system of government into a democracy." (Lloyd George 1923, 182.)

<sup>41</sup> This negative result is not due to the fact that we rely on data on the *end* of wars rather than data on *ongoing* wars. We find the exact same result if we interact conscription with a war dummy.

<sup>42</sup> Boix et al., 'Dataset of Political Regimes'.

<sup>43</sup> Przeworski, 'Political Institutions'.

<sup>44</sup> Canby, *Military Manpower Procurement*, 43.

<sup>45</sup> Huntington, *The Soldier and the State*, 38.

<sup>46</sup> Nassif, 'Coups and Nascent Democracies.'

**Table 1: Descriptive Statistics**

	Mean	SD	Min	Max
<i>Extensions of Male Franchise</i>				
Extension (male)	0.03	0.17	0	1
Conscription <sub>t-1</sub>	0.87	0.34	0	1
War <sub>t-1</sub>	0.14	0.35	0	1
ln(GDP per Capita) <sub>t-1</sub>	7.65	0.53	6.43	9.03
Urbanization <sub>t-1</sub>	15.20	8.60	2.10	47.57
Economic Growth <sub>t-1</sub>	1.91	4.89	-22.43	26.11
Family Farms <sub>t-1</sub>	20.72	13.56	1	60
Resource Dependence <sub>t-1</sub>	1.97	4.65	0	41.56
Time <sub>t-1</sub>	45.03	31.12	0	151
Previous Extensions	1.98	1.14	0	4
<i>Extensions of Female Franchise</i>				
Extension (female)	0.02	0.15	0	1
Conscription <sub>t-1</sub>	0.87	0.34	0	1
War <sub>t-1</sub>	0.16	0.37	0	1
ln(GDP per Capita) <sub>t-1</sub>	7.82	0.61	6.43	10.34
Urbanization <sub>t-1</sub>	15.52	8.64	2.10	62.61
Economic Growth <sub>t-1</sub>	1.87	6.48	-24.92	47.86
Family Farms <sub>t-1</sub>	24.40	16.74	1	90
Resource Dependence <sub>t-1</sub>	3.46	10.71	0	100
Time <sub>t-1</sub>	57.88	40.48	0	161
Previous Extensions	0.22	0.44	0	2
<i>Transitions to Democracy</i>				
Democracy	0.02	0.16	0	1
Conscription <sub>t-1</sub>	0.65	0.48	0	1
War End <sub>t-1</sub>	0.02	0.15	0	1
ln(GDP per Capita) <sub>t-1</sub>	7.57	0.77	5.83	10.34
Urbanization <sub>t-1</sub>	19.50	15.21	1.27	102.16
Economic Growth <sub>t-1</sub>	1.69	6.63	-61.49	66.49
Family Farms <sub>t-1</sub>	33.06	20.26	0	84
Resource Dependence <sub>t-1</sub>	6.47	12.68	0	100
Time <sub>t-1</sub>	58.29	53.03	1	205
Previous Democracy	3.34	8.77	0	84
<i>Transitions to Autocracy</i>				
Autocracy	0.02	0.13	0	1
Conscription <sub>t-1</sub>	0.64	0.48	0	1
ln(GDP per Capita) <sub>t-1</sub>	8.55	0.88	6.04	10.34
Urbanization <sub>t-1</sub>	25.22	14.53	1.73	90.48
Economic Growth <sub>t-1</sub>	2.24	4.77	-23.00	65.90
Family Farms <sub>t-1</sub>	49.05	25.02	0	98
Resource Dependence <sub>t-1</sub>	2.35	5.01	0	90.44
Time <sub>t-1</sub>	39.91	40.47	1	206
Previous Authoritarianism	63.61	55.96	0	192
<i>Coups</i>				
Coup	0.01	0.11	0	1
Conscription <sub>t-1</sub>	0.65	0.48	0	1
ln(GDP per Capita) <sub>t-1</sub>	8.56	0.88	6.04	10.34
Urbanization <sub>t-1</sub>	25.33	14.58	1.73	90.48
Economic Growth <sub>t-1</sub>	2.24	4.76	-23.00	65.90
Family Farms <sub>t-1</sub>	49.23	25.06	0	98
Resource Dependence <sub>t-1</sub>	2.38	5.04	0	90.44
Time <sub>t-1</sub>	42.56	36.51	0	157
Previous Coups	0.23	0.64	0	4

**Table 2. Franchise extensions, men.**

	(1)	(2)	(3)
Conscription <sub><i>t</i>-1</sub>	-0.29 (0.35)	-0.74** (0.36)	0.33 (1.19)
War <sub><i>t</i>-1</sub>		-0.97** (0.46)	-0.13 (0.98)
Conscription <sub><i>t</i>-1</sub> × war <sub><i>t</i>-1</sub>		2.38*** (0.53)	2.00** (0.96)
GDP per capita (logged) <sub><i>t</i>-1</sub>			1.07* (0.56)
Urbanization <sub><i>t</i>-1</sub>			0.02 (0.02)
Economic growth <sub><i>t</i>-1</sub>			-0.07 (0.05)
Family farms <sub><i>t</i>-1</sub>			-0.02 (0.02)
Resource dependence <sub><i>t</i>-1</sub>			0.04* (0.02)
Time	-0.00 (0.04)	-0.00 (0.04)	-0.02 (0.05)
Time <sup>2</sup>	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Time <sup>3</sup>	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Previous extensions	-0.34** (0.15)	-0.34** (0.15)	-0.45* (0.24)
Observations	1371	1366	837

Note: Entries are logit coefficients with robust standard errors, clustered on countries, in parentheses. Dependent variable: franchise extensions for males (0, 1).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 3. Franchise extensions, women.**

	(1)	(2)	(3)
Conscription <sub><i>t-1</i></sub>	-1.03*** (0.32)	-1.18*** (0.32)	-0.52 (0.47)
War <sub><i>t-1</i></sub>		-1.01 (1.36)	-0.38 (1.46)
Conscription <sub><i>t-1</i></sub> × war <sub><i>t-1</i></sub>		1.36 (1.44)	0.29 (1.51)
GDP per capita (logged) <sub><i>t-1</i></sub>			-1.02** (0.42)
Urbanization <sub><i>t-1</i></sub>			0.08*** (0.03)
Economic growth <sub><i>t-1</i></sub>			0.02 (0.03)
Family farms <sub><i>t-1</i></sub>			0.05*** (0.01)
Resource dependence <sub><i>t-1</i></sub>			0.01 (0.01)
Time	-0.03 (0.03)	-0.03 (0.03)	-0.09*** (0.03)
Time <sup>2</sup>	0.00 (0.00)	0.00 (0.00)	0.00*** (0.00)
Time <sup>3</sup>	-0.00 (0.00)	-0.00 (0.00)	-0.00*** (0.00)
Previous extensions	0.53 (0.41)	0.53 (0.42)	-0.05 (0.57)
Observations	2506	2500	1540

Note: Entries are logit coefficients with robust standard errors, clustered on countries, in parentheses. Dependent variable: franchise extensions for females (0, 1).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 4. Transitions to democracy.**

	(1)	(2)	(3)
Conscription <sub><i>t</i>-1</sub>	0.08 (0.22)	0.11 (0.24)	0.11 (0.24)
War <sub><i>t</i>-1</sub>		0.82 (1.07)	1.02 (1.10)
Conscription <sub><i>t</i>-1</sub> × war <sub><i>t</i>-1</sub>		0.13 (1.17)	-0.01 (1.26)
GDP per capita (logged) <sub><i>t</i>-1</sub>			0.51** (0.22)
Urbanization <sub><i>t</i>-1</sub>			-0.00 (0.01)
Economic growth <sub><i>t</i>-1</sub>			-0.03* (0.02)
Family farms <sub><i>t</i>-1</sub>			0.00 (0.01)
Resource dependence <sub><i>t</i>-1</sub>			-0.06*** (0.02)
Time	-0.03** (0.01)	-0.03** (0.02)	-0.04*** (0.02)
Time <sup>2</sup>	0.00* (0.00)	0.00* (0.00)	0.00** (0.00)
Time <sup>3</sup>	-0.00* (0.00)	-0.00* (0.00)	-0.00** (0.00)
Previous transitions	0.03*** (0.01)	0.03*** (0.01)	0.01 (0.01)
Observations	6317	6123	3875

Note: Entries are logit coefficients with robust standard errors, clustered on countries, in parentheses. Dependent variable: democracy (0, 1).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 5. Transitions to authoritarianism, coups**

	(1)	(2)	(3)	(4)
Conscription <sub><i>t</i>-1</sub>	0.02 (0.25)	0.02 (0.30)	-0.14 (0.45)	0.26 (0.37)
GDP per capita (logged) <sub><i>t</i>-1</sub>		-0.90*** (0.27)		-0.85*** (0.31)
Urbanization <sub><i>t</i>-1</sub>		-0.01 (0.02)		-0.02 (0.02)
Economic growth <sub><i>t</i>-1</sub>		-0.06** (0.03)		-0.02 (0.02)
Family farms <sub><i>t</i>-1</sub>		-0.02** (0.01)		-0.02* (0.01)
Resource dependence <sub><i>t</i>-1</sub>		0.00 (0.02)		0.00 (0.02)
Time	-0.05 (0.04)	-0.01 (0.05)	-0.04 (0.07)	0.00 (0.07)
Time <sup>2</sup>	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Time <sup>3</sup>	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Previous transitions/coups	0.00 (0.00)	0.00 (0.00)	0.27** (0.12)	0.51*** (0.17)
Observations	4056	3132	3353	3081

Note: Entries are logit coefficients with robust standard errors, clustered on countries, in parentheses. Dependent variable: autocracy (0, 1) (models 1-2), coups (0, 1) (models 3-4).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$